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Discussions

Interactive comment on "Uncertainty and sensitivity in optode-based shelf-sea net community production estimates" *by* T. Hull et al.

Anonymous Referee #1

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Review of Hull et al. Uncertainty and sensitivity in optode-based shelf-sea net community production estimates. Biogeosciences Discuss., 12, 15611–15654, 2015

This paper uses data from an autonomous mooring in the southern North Sea to estimate net community production using a 0-dimensional mixed layer oxygen mass balance model. The manuscript rightly points at the importance and difficulty for constraining NCP in productive and highly variable shelf systems and uses an interesting and useful approach, with potential for long term monitoring of this important property of ecosystems. Making algorithms available as open source tools is valuable and promotes usability. The paper is well written and I would like to praise the frank and thorough analysis of uncertainty. However I have some minor comments and one fundamental question related to what the model is estimating that I would like to see

discussed before recommending publication.

P2, L19-20: Although ultimately connected to the atmosphere, I would say that photoautotrophic marine organisms fix CO2 from the sea.

P3, L11-12: "NCP makes no distinction between imported DIC and locally respired carbon." I find this sentence confusing. Do it refers to NCP as ecological property (i.e. the balance between auto- and heterotrophic processes in a community, which in my view does make such distinction), or as measurable (and hence method-dependent) metric? This distinction is important (see my last comment below) and should be made clear throughout.

P4, L1-3: Bottle samples may, but also may not be spatially disparate from the source of production. It depends on the design of sampling. E.g., there is no reason why sampling based on inspection of a vertical profile of Chla fluorescence should miss the DCM. This is precisely what Weston et al. (2005) did to determine the contribution of (bottle incubations derived) PP in the DCM; so I would say that this reference goes against the author's statement that "bottle samples may be spatially disparate from the source of production, for instance where deep chlorophyll maxima form, and thus not capture the organisms of interest". Actually Weston et al. (2005) state: "These subsurface blooms are not detectable using satellite or air borne remote sensing, with in situ observation currently the only way of detecting phytoplankton biomass.", where in situ observation means 14C bottle incubations.

Please revise the list of authors in Weston et al. (2005).

P.4, L15-16: Validated satellite based productivity algorithms for NCP have already been published (Tilstone et al. 2015. Remote sensing of Environment 164, 254-269). This paper also mentions previous regional to global estimations of NCP from empirical 14C-PP:NCP relationships and satellite-based estimates of PP (Duarte et al., 2001; Serret et al., 2001; Serret, et al., 2002, Chang et al. 2014, Nevison et al. 2012, Westberry et al, 2012).

P.6, (2.1. Study site). As the discussion states that "processes occurring at or in the sea bed are incorporated into the mixed layer mass balance and thus the NCP estimate", I would add the corresponding information to the description of the study site ("the Warp is vertically well mixed"), so that the reader may have this into account through the results section.

P.7, L.8-12. My understanding is that only ca. 7% of the whole dataset (245 (150+95) out of 3650 days) was used because of the poor quality of the measurements because of biofouling. It is unclear whether these periods were chosen because of their extent, nor how many data were discarded. As there seems to be strong interannual variability, with the chosen periods of 2008 showing the highest positive and negative NCP values compared to other years (Figs. 3a and A3), and given that the gaps in the 2008 time series are subsequently filled with data from other years, could the modeling be run with the mean 10 years oxygen fields, for comparison?

P.14, L17-18. The chlorophyll data should be expressed in mg Chla m-3, instead of fluorescence readings. This is necessary to compare phytoplankton abundance with metabolic rates.

P.17, (4.1 NCP). The calculation of 50% contribution of sedimentary processes to negative NCP estimations goes against the first assumption of the model: "This describes the oxygen mass balance in the mixed layer assuming no vertical or horizontal advection and no turbulent diffusion across any mixed layer boundary." (P.8 L6-8). I think that the paper should explain what are the consequences of the study site not meeting this important assumption; specifically I think that the paper should explicitly state what ecological/biogeochemical variable the model is exactly estimating here, i.e., which is the "community" whose "net production" is estimated. Both the text and references in the Introduction seem to indicate that NCP is planktonic, or at least pelagic. However, if sedimentary processes contribute 50% to NCP estimations by the model, surely these estimations are not of planktonic NCP. This is not only a matter of the community (i.e. which are the organisms involved and where/how they live and interact) but the

processes. As the authors adequately refer, "nitrification and the oxidation of reduced material other than ammonia and nitrite" are important oxygen consuming processes in shelf sediments. But these oxidations are typically associated to chemoautotrophy and the reduction of carbon dioxide as carbon source for growth, i.e. in any metabolic balance calculation for the water column + sediments, these oxygen-consuming processes would sum up at the autotrophic side. However, in the O2 based model, they contribute as negative NCP. One difficulty is that the impact and scale of benthic (and possibly riverine) and planktonic processes are not the same nor constant, leading the system of study far from steady state, which makes it difficult to determine which is the variable contribution of each component to the overall O2 balance. For example, according to Figs.2c and 3a, all the negative NCP episodes during the winter occur whenever the wind speed exceeds 10 ms-1, which suggests a physical rather than planktonic control of O2 deficiency ("NCP") in the water column, maybe through resuspension/mixing. High wind speed episodes also coincide with decreases in NCP during and after the spring bloom, which complicates ascertaining which is the planktonic metabolic balance during the bloom and how large is the contribution of planktonic heterotrophic processes to the post bloom decrease in the O2 budget. In any case, the same name (NCP) seems to be being given to the balance of oxygen photosynthesis and aerobic respiration of a planktonic community and to the mixing of O2 deficits accumulated in the sediments after auto- and heterotrophic oxygen-consuming processes. A significant contribution of chemoautotrophy to episodes of negative "NCP", or to the overall oxygen consumption could compromise the conclusion that this is a region of net heterotrophy, and should be at least discussed. Surely many reduced materials used in these oxidations are connected to the decomposition and ultimately to the production of organic matter elsewhere (river, water column, etc.), but these processes would balance over scales much longer and larger than what reflects the variation in mixed layer O2 budgets. The description of the study site (P.6) indicates that riverine inputs are significant, which should also impact a) directly on the O2 budget and b) indirectly on the metabolic response of both plankton and benthos. I think that the influence of riverine inputs on both the annual balance, temporal variability and uncertainty should be discussed.

Interactive comment on Biogeosciences Discuss., 12, 15611, 2015.